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LOGICAL LEARNING AND RETENTION: A GENERAL REVIEW OF EXPERIMENTS WITH MEANINGFUL VERBAL MATERIALS

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I. Introduction

Throughout the brief but extensive history of memory investigation, we find pleas for more attention to memory as it occurs in actual life. (For an example, cf. Meumann, **55**). Of late there is increasing evidence that this appeal is being answered. One result is an emphasis upon " logical " memory or " memory for substance " in contrast with the serial learning of nonsense syllables. It now seems evident that any theory of learning is unlikely to prove acceptable unless it is based on investigations dealing with meaningful material. These investigations, however, have not been systematically reviewed as such since Whipple's material (**75**) was published in 1915. To make such a review is the purpose of this paper. We have selected from the considerable mass of published material those studies which help to bring to light the nature of " logical " memory.

By " logical " memory we mean memory for connected material measured in terms of the " substance " remembered. Logical memory is generally regarded as one end of a series at the other end of which is " rote " memory for purely nonsensical, entirely disconnected material. In this review we shall include some references to studies which contrast memory for highly meaningful material with rote memory even though they do not all measure memory in terms of

substance alone. We have, however, resisted the temptation to include studies of non-verbal learning, for until more secure interpretative formulae have emerged from the facts it is difficult to integrate data secured by too widely differing techniques.

II. Significant Differences Between Logical and Rote Memory

Ease in Learning. It has probably always been recognized that connected material may be learned more quickly than unconnected material of the same general character, though the fact has influenced learning theory strangely little. Earlier experimenters, such as Binet and Henri (11), obtained data showing greater ease in the learning of connected material, but most of these experimenters used a strictly verbatim criterion of learning. It is not easy to make a direct comparison between verbatim reproduction and logical memory in which only "ideas" are recalled. In logical memory percentages of correct reproduction obviously depend upon the scoring system and, if the recognition method is used, upon the degree of divergence between the original and the test items. To this must be added differences in the material learned, both as to content and form. The last were overcome by Jones and English (42) by measuring both logical and verbatim memory with the same subjects and the same passage. A test for "ideas" retained after one reading was followed by learning to the point of verbatim mastery. Ninety-one words of simple narrative were treated as yielding 31 "ideas." After one reading from 35% to 93% of these ideas were reproduced (with a mean of 76%). To master the ninety-one-word narrative verbatim required from 2 to 9 additional repetitions (mean 5.3). It may be added that other subjects, not reported above, failed entirely to master the verbatim task within appointed time and coöperative-tolerance limits, yet made nearly average scores in logical memory. It seems fairly clear, then, that just as verbatim learning of unconnected but meaningful material is easier than the learning of nonsense material and in turn as connected material is easier to learn verbatim than unconnected material, so the "ideas" are easier to learn than the exact words.

Retention. Comparison between logical and rote memory is more directly feasible with respect to retention since we can use immediate memory as a base for computing loss (or gain) at various intervals thereafter. Numerous studies furnish evidence contributing to such a comparison. We shall consider first a group of studies dealing

solely with logical memory. Despite variations in material and procedure, these studies show an unmistakable trend in the direction of better retention than the classical curve of Ebbinghaus shows.

Whipple (**75**) summarized earlier data showing that retention of connected material is better than that of discrete impressions. Henderson (**37**), for example, found that only 8% to 15% of his material was forgotten in 4 weeks. Yoakam (**83**) had pupils in grades 4 to 8 read a passage once and take tests of multiple choice and completion types. The mean score for all groups was 38.9% on immediate recall and 33.9% after 20 days, a loss of only 12.8%. Peterson (**58**) had 56 college students study a 250-word passage for 2½ minutes. By the reproduction method and scoring on "ideas," there was a loss after 1 week of only 15% of the immediate score. Greene (**34**) made a careful investigation using a large group of college students as subjects. With 1 presentation, the immediate retention was 53.1%, while that 1 week later was 41.2%, a loss of only 22.4%. Bassett (**9**) in a school testing program where the criterion was largely free from verbatim requirements obtained percentages of retention ranging from 86% at 4 months to 72% at 16 months.

The study of Dietze and Jones (**19**) is noteworthy because of its careful technique. Using prose passages over 1,000 words in length,

TABLE I

RETENTION SCORES OBTAINED BY THREE MEMORY STUDIES

Interval	Dietze & Jones (Prose)	Ebbinghaus (Syll.)	Radosawljewitsch (Poetry)
Immediate	56.7%	58.0%	96.0%
1 day	48.0%	34.0%	79.0%
14 days	36.3%	22.0%	30.0%
30 days	32.4%	21.0%	24.0%
100 days	30.0%		

presented once, large groups of school pupils as subjects, and highly reliable multiple choice tests, they obtained the data shown in Table I, together with other data for comparison.

The losses in terms of percentage of immediate retention between the immediate testing and that of the thirtieth day in these 3 studies are respectively 43%, 63.8%, and 75%. It thus appears that material read once and scored for substance was retained better after 30 days than other materials which had been repeated many times. Dietze and Jones, moreover, have devised a method of treating their data whereby all extraneous factors are supposed to be eliminated and the amount of actual forgetting determined. According to this method the retention at the indicated intervals was actually 90%, 76.2%, 59.3%, 51.7%, and 47.6%. It is of course not legitimate to compare these calculated percentages with those of other studies because none employs a similar tech-

nique, but in themselves they suggest that loss of substance is very gradual—certainly very much more gradual than the loss in case of rote memory. Indeed Trow (72) obtained a rise in retention score during 15 weeks, but he used only 2 subjects.

While the studies dealing solely with logical memory show in a general way that retention is much greater than in the case of rote memory, a number of other experiments have been carried out in such a way as to include both logical and verbatim memory in the same experiment. These supply material for more careful and exact comparison.

Sharp (64) and Henderson (37) found that words are forgotten to a greater extent than ideas. Greene (34) found that at the end of a week test items requiring an inference showed somewhat better retention than those requiring factual memory. Tyler (73) in a very careful study, obtained analogous results. Ability to answer questions demanding inference showed a much smaller loss over long periods than ability to recall or recognize factual material. Indeed in the former there was frequently a definite gain. Grant (33) showed that connected material is forgotten to a less degree than unconnected material. McGeoch and McKinney (53, 54) found incidental evidence of a difference between substance and verbatim memory in the 2 studies of retroactive inhibition in poetry and prose. Verbatim reproduction of poetry showed losses in 7 days greater in some cases than 50%, but retention in terms of combined verbatim and substance reproduction, a definite criterion for substance reproduction being used, was about 16% greater at the end of 7 days than immediately after learning. The loss in prose substance was only approximately 8% in the same interval.

English, Welborn, and Killian (21) made extensive comparisons of verbatim and substance memory for prose passages over 1,500 words in length. The procedure was unique in that comparison was made, not of 2 learning tasks as in other studies, but of the relative effectiveness of verbatim and substance memory in a single learning task. In the principal series of experiments the subjects read a passage and took a test of the true-false recognition type at various intervals. Two kinds of items were included in a single test: (1) verbatim items, which were practically perfect reproductions of statements from the passage, employing the vocabulary of the passage throughout; and (2) summaries or topic sentences employing a different vocabulary and covering sections of varying length in the passage itself. Separate scores were obtained for the 2 kinds of items. At no time were the subjects aware of the real purpose of the experiments or of the existence of 2 kinds of items. Throughout it was shown that during an interval of 4 to 14 weeks there was a consistent difference in median retention of the 2 kinds of items.

While the verbatim items showed moderate loss, the summary items showed either no loss or an actual gain. There were 2 independent investigations using different passages and tests, but the differences were essentially the same in all cases.

In a follow-up study English (22) computed not the median scores but the percentage of subjects whose scores indicated improvement in later trials over the immediate trial. Statistically reliable differences between the summary tests and the verbatim tests were again found.

The trend of these investigations clearly is that the curve for substance memory differs radically from that for rote memory. A few investigators contend, on the other hand, that the curve for substance memory is essentially similar to the Ebbinghaus curve.

Austin (6), who made a very extensive study, reported rapid loss in substance memory at first, but little loss between 2 and 4 weeks. She considered her results to be similar to those of Ebbinghaus. Jones (41) reported immediate retention, 62%, and fourteen-day retention, 30.6% as compared with 58% and 22% in Ebbinghaus' experiment. Davis and Moore (15) reviewed 61 memory studies and reached the conclusion that the curve of retention has the same general character in the case of nonsense and meaningful material. Unfortunately the studies are not listed so that we can determine whether the studies of meaningful material accepted "ideas" or demanded verbatim reproduction.

The number of important studies reporting data comparable with those of nonsense syllables is in fact still too small for any safe conclusion, especially in view of the great variation in procedure. Indeed, as Carr (13) has pointed out, there are as many curves as there are learning variables. There is none the less discernible a strong trend toward the conclusion that substance memory retention has a radically different dynamics from that of rote memory.

Correlational Data. Studies designed to throw light on the question of a general memory factor may first be reviewed for the differences which they reveal or do not reveal between logical and rote memory.

Woodrow (82) obtained intercorrelations of 6 memory tests, one of which was a test of substance memory, and found that the mean coefficient was .38. Johannsen, Stirling, and Levine (40) correlated scores in like and unlike pairs of school subjects and found some basis for stating that memory ability is specific for the type of material learned. Garrett (26) administered 8 memory tests to 158 students in Columbia College. The intercorrelations were very low and suggested that the tests dealt with relatively specific abilities. By the Spearman tetrad-difference technique he found a small but unreliable general memory factor. Anastasi (3) with a comparatively narrow range of tests

obtained evidence of a general memory factor. The same experimenter (4) later employed a wider range of tests for the same purpose. Her data are too extensive for brief summary, but we shall quote her conclusion: "The common factor previously found through a certain type of memory test cannot be regarded as a general memory factor extending through all forms of memory" (p. 45).

The second line of evidence deals directly with the degree of correlation between substance memory and various kinds of rote memory.

Wissler (81) correlated memory for a hundred-word passage with memory for digits and colors. For like pairs (*i.e.* prose with prose, or unconnected with unconnected) he obtained r's of .29 and .39, while for unlike pairs the r's were .03 to .05. Bennett (10), using various materials and only 9 subjects, obtained a mean coefficient between unconnected and connected material of —.01. Kitson (46) obtained a coefficient of .09 between logical material and numbers. Jones and English (42) obtained a coefficient of .328±.08 between scores on ideas from a story read once and number of repetitions of the same story for verbatim learning. (In this experiment the attenuating effect of different materials upon the correlation was avoided; only the criterion of "memory" differed in such a way as to contrast substance and rote memory.) Anastasi's work (4) along this line is probably more thorough than any other. She obtained a coefficient of .647 (corrected for attenuation .74) between 2 tests of substance memory, but between substance memory and memory for syllables she obtained coefficients of .154 and .094. Welborn (74), whose study has already been described, obtained coefficients for pairs of similar tests ranging from .664±.04 to .884±.02, but for dissimilar pairs coefficients ranging from .419±.06 to .589±.04. Killian (44) obtained a coefficient of .39 in case of dissimilar pairs with a test having a reliability coefficient of .69. Smith and McDougall (67) contrasted the correlation of substance and nonsense syllable learning with the correlation of 2 forms of purely rote memory. In the 2 cases the coefficients were respectively .48 and .61.

The trend of all the correlational data may thus be fairly interpreted as demonstrating that different factors operate in substance and rote memory.

Other Differences. Fox (24) in reviewing certain experiments concluded that the enormous difference in the number of repetitions required to learn nonsense and meaningful material is in itself an indication of the disparity between the two. Killian (44) obtained some evidence, though inconclusive, that memory for his verbatim and summary items was differently affected by the number of repetitions.

Winch (79) studied the transfer of training in rote memory for groups of letters to substance memory for stories. His results are

inconclusive, but the study is mentioned as an example of the many possible ways of getting at the issue.

III. Form or Organization as the Basis of Substance Learning

The influence of the total meaning or pattern seems to have been noted first by Henderson. He concluded that the fact that all the topics are vaguely in the mind at once is "quite different from mere mechanical association."

Lewis (**49**) found that memory for hundred-word legends is largely a matter of the persistence of organized wholes. In his material he included irrelevant details to the extent of 37.5%; they accounted, however, for 63% of the omissions in recall. Laird, Remmers, and Peterson (**47**) found that in case of learning Anglo-Saxon words, additions, and dates, logical grouping gave much better results than learning in random order, that organized recall was better than unorganized, and that the balance in favor of organization became greater with the lapse of time. Key (**43**) also found that organization is an important factor. She showed that organization in memory includes two factors, viz., typification and unification. By the first she means that the essential details stand out prominently and that associations relating to the form as a whole tend to be retained. By the second she means that details are gathered into groups or units. Guilford (**35**) compared memory for number series that had plan or form with that for series without plan; also word lists that could be made into sentences with those that could not. His data showed a reliable difference in favor of material possessing form or pattern.

Aside from total pattern, detailed meanings, relations, inferences, etc., have been found to aid substance memory.

Foster (**23**) read a number of stories repeatedly to small children. By the use of the method of hesitating for the child to supply the next word, she found that correct response depended little on the familiarity of the words, but considerably on the closeness of relationship between the last word read and the one requested. Key (**43**) found that learning of meaningful material was 20 times as great as learning of non-meaningful material. Reed (**61**) studied the influence of associations in learning various types of material, and showed that easy prose such as *The Marble Statue* is learned far more quickly (111 sec. *vs.* 261 sec.) and retained much better (39.5 *vs.* 4.00 at the end of 2 weeks) than an abstract passage such as *The Origin of Ideas.* He concluded that the most valuable type of association is that based on meaning. Substance memory seems to be aided by reasoning ability. For example, Peterson (**57**) obtained a correlation coefficient of .40 between substance memory for passages and generalization and reasoning tests. Moreover, substance memory appears to be related to inventiveness. Winch (**80**) studied the effect of practice in memorizing short stories on ability to invent stories from a given list of words and found some transfer not due to common elements of material. The 2 abilities correlated from .276 to .749. Arnold (**5**) studied directly the factor

of association in memory for poetry and obtained evidence alleged to be incompatible with associationistic principles.

It has been found in a large number of studies that with the lapse of time various types of change occur in memory for substance.

Henderson (37), Austin (6), and Bartlett (8) noted that the material becomes more condensed and generalized with loss of original wording. Shaw (65), Binet and Henri (11), Henderson (37), and Lewis (49) found also that the accessory parts and accidentals are lost to a much greater degree than the fundamental ideas. But even more striking changes have been found. Binet and Henri (11) noted verbal assimilation, *i.e.* simplifying by substituting familiar for unfamiliar words. Construction or introduction has been found by Henderson (37), Key (43), Bartlett (8), Müller-Freienfels (56), and others. Henderson also found regrouping and simplification.

IV. Relation of Substance Memory to Other Variables

Intelligence of the Subject. Several studies show that the more intelligent subject tends to do better than the less intelligent. Simpson (66) found that only 10% of a poor group surpassed the lowest 23% of a good group. English (20) with a somewhat similar procedure found that on prose passages, the superiority of a group of highly intelligent and socially advantaged English boys over an inferior group was "almost overwhelming." With verbatim recall of unconnected words the superiority of the advantaged group was less pronounced. Within each group, moreover, substance memory showed a considerably higher correlation with estimated intelligence.

Dietze (17) obtained mean coefficients between M.A. and memory score ranging from .55 for immediate tests to .25 for tests at 100 days. Wilson (77) compared retention of a story by dull nine-year-old, bright nine-year-old, dull twelve-year-old, and bright twelve-year-old children. The curves were practically parallel, the bright nine-year-olds and dull twelve-year-olds having practically identical curves in a middle position and the bright twelve-year-olds having one somewhat above and the dull nine-year-olds one somewhat below the middle curves. Wilson (78) obtained some evidence that brighter children do better with abstract and more difficult ideas than duller children. Grant (33) gave 10 memory tests, 3 of which were based on connected material. The coefficients between these three and intelligence were higher than those of any of the other tests. Bassett (9), in the case of retention of history, obtained a mean correlation with M.A. of .366. Greene (34) found that the upper quartile obtained their best scores after individual reading, while the lower quartile did best after lectures. With college students Garrett (26) obtained a coefficient of only .29 for logical memory *vs.* intelligence, but Foster (23) obtained a high correlation between M.A. and substance memory in young children. Good (32) found a greater difference between the upper and lower quartiles in performance requiring thought than in reproduction of ideas. Bolton (12) found a

close relation (r=.695±.025) between logical memory and intelligence while with the same 50 students the relation between "retentivity" and intelligence was only .065. Hegge (36), working with 98 subnormals at Vineland, obtained a correlation of .757 between logical memory and intelligence.

Rapidity of Learning. Lyon (51), Pyle (59), Henderson (37), Gates (29), Peterson (58), and Stump (70) found evidence that rapid learners retain connected material better than slow learners. Peterson found also that the advantage of the rapid learner increases in proportion to the difficulty of the material. The difference found by Stump is striking. Rapid learners lost little in 42 days, while slow learners lost as much as 60%. The correlation between rate of learning and amount retained was .71.

Scholarship. Wissler (81), Henderson (37), Pyle (59), Simpson (66), and Travis (71) obtained rather low correlations between substance memory and scholarship. King and Homan (45) obtained a number of coefficients with several groups and materials. The highest coefficient for any group was .38. They assert that logical memory is a "fair measure" of school standing. Pyle suggests that substance memory does not correlate very highly with scholarship because numerous other variables, especially study habits, influence scholarship.

Age. Henderson (37), Shaw (65), King and Homan (45), Pyle (60), and Dietze and Jones (19) present evidence that substance memory scores increase from the earliest ages tested to puberty.

Foster (23) obtained a correlation of .74 in case of a group of children having C.A.'s from 2-7 to 4-9, but for subjects in grades 7 to 12 Dietze obtained low r's between C.A. and retention scores, the mean coefficients ranging from —.02 to +.24. Henderson and King and Homan found evidence that the increase continues through college. On the other hand Garrett (26) obtained "substantially 0" correlation in case of a group 16 to 26 years old, and Lodge and Jackson (50) found that college students under 25 did better than those over 25. Shaw and Henderson found that on the basis of later reproductions younger subjects retain as well as adults. King and Homan found that the per cent who do better on the second than the first reproduction increases from 19 in elementary school to 45 in college.

We may accept Dietze's conclusion that factual memory (by which he means substantially what we have called logical memory) shows decided increase from early life to puberty and that it increases more slowly from puberty to maturity.

Sex. Shaw (65) found girls superior to boys from the third grade to college; Pyle (60) found the same true from 8 to 15 years of age. On the other hand Foster (23) found boys consistently superior in case of her group ranging from 2-7 to 4-9 years of age, and Dietze (18) found that in a large

group in grades 7 to 12, when comparison was made by age groups, the boys did better. Pyle (60) found that the scores of boys increase up to 18 while those of girls increase up to only 15 years of age. Studies by Wissler (81), Gates (27), Lodge and Jackson (50), and Travis (71), agree on the point that women are superior to men in substance memory. In none of these studies, apparently, was the factor of physiological age held constant.

Material. Johannsen, Stirling, and Levine (40) correlated memory for literature, history, and science and concluded that memory ability is specific for the type of material. Reed (61) found that material with familiar associations has an advantage both for learning and retention. Binet and Henri (11) found that with increase in length of the material the number of words reproduced increases, but not in direct proportion. Also, there were more substitutions than omissions in the case of short sentences, but the reverse was true in the case of long sentences.

Serial Position. Dell (16) and Wilson (78) found that this factor is negligible in case of connected material. On the other hand Shaw (65) showed by various calculations that memory for the first part of a story, particularly the first sentence, is better than for the latter part. Also, Jersild (39) found that primacy has a high value in memory for a biography.

Mode of Presentation. Greene (34) found no significant difference in the case of college students between lectures and reading on immediate tests, but on retention tests at the end of one week, there was a highly reliable difference (99 chances out of 100) in favor of lectures. Kitson (46) obtained a coefficient of only .26 between logical material heard and seen. Russell (62) found that the advantage shifts from oral presentation in the fifth grade to silent reading in the ninth.

Feeling Tone and Temperament. A considerable number, perhaps a majority, of the studies of hedonic tone have reported substance memory, although seldom with much attempt to separate it from rote memory. These studies have been reviewed by Meltzer (54a) and need not be repeated here.

Attitude of the Learner. Fox (24) concluded from a review of experimental data that the desire or will to remember is the essential determining factor. Aall (1) tested memory for 2 stories and found a mean advantage of 13% for the intention-to-remember group. We have not had an opportunity to analyze Aall's data. Carr (13) expresses the conviction that in most memory experiments the amount recalled is a function of the expectation to be tested. Jones (41) compared retention of college lectures in case groups with and without tests immediately after learning. The effect of the tests was to keep the retention curve practically horizontal for 4 days; thereafter the decrement was slow during 8 weeks, at the end of which time those who took the immediate test did almost twice as

well as those who did not take the test. Bassett (9) obtained a mean correlation of .664 between interest in, and retention of, history. Fox (25) in a comparison of the whole and "mixed" methods of memorizing poetry found that the preference of the subject for certain of the poems had greater influence than the method used.

Guidance. Greene (34) compared guided reading with unguided reading and lectures by having the experimental group mark the test questions while they studied. On inference tests the retention score was 62%, while that for the control group was 10%. On information tests the per cent was 64 against 46. These differences were highly reliable.

Germane (30) found that reading preceded by a list of questions is better than rereading or reading followed by making a summary, and Yoakam (83) found that an initial test aids permanent retention. A test given before the completion of learning may be regarded as a form of guidance. Welborn (74) gave such tests and found a fairly reliable advantage in 7 comparisons out of 8. The familiar recitation method may possibly be interpreted as guidance. Gates (28) studied the effect of recitation on memory for biographies and found that for the optimal division of time (60% devoted to recitation) on immediate tests the difference in favor of the recitation method was 26.6% and on retention tests 3 or 4 hours later, 49.7%. Trow (72) found in a series of practice periods that presentation preceded by recall gave better results than several other procedures.

Number of Repetitions. While the evidence is neither extensive nor conclusive, it indicates that repetition is not of major importance in logical memory.

Killian (44) compared scores based on various numbers of readings from 1 to 4 and found small, but inconclusive, differences in favor of 3 readings. Good (31) found that 2 readings are not much more effective than 1 reading, and Henderson (37) found that a single attentive reading of a passage (135 to 180 words) is as effective as 3 minutes of study. Shaffer (63) and Jersild (38) have shown that an increase of scores occurs with repeated readings up to 4 and 5 respectively, but under conditions that raise doubt as to the true causes. Shaffer's subjects took a pre-test and a test following each reading. Jersild's results are difficult to evaluate because of the large number of variables in his experiment, but taken at face value, they do not show conclusive advantages for repetition. He concludes that "one of the least effective forms of emphasis is to repeat an item following its first presentation" and that "the benefit does not increase in proportion to the number of added repetitions" (p. 70). From the standpoint of study methods, Yoakam (83) and Dietze and Jones (19) proved that a single reading of a lesson is very inefficient.

In view of the Gestalt contention that frequency operates merely to make possible the acquisition of insights or meanings previously

not apprehended, it would be illuminating to have some of these studies analyzed item by item after each repetition. So far as we know, this has not been done with substance learning, although it is more appropriate for such a check than nonsense learning.

Spacing. The data for verbatim memory, which are fairly extensive, do not come within the limits of this review. Three careful studies of substance memory show an advantage for spaced learning.

Foster (23) compared 5 readings a day for 2 days with daily readings and found that the daily method gave better results especially in earlier reproduction. Austin (6) found that daily repetitions are better than 5 a day and that in a range from 5 per day to one every fifth day, daily and alternate daily repetitions are best, but contrary to Foster, she found that the advantage shows more on retention than immediate tests. Welborn (74) with 4 repetitions had spacings between them ranging from none (*i.e.* 4 consecutive repetitions) to 1 repetition every third day. He found that three-hour and daily spacing gave the best results.

Reminiscence.[1] Williams (76) and McGeoch (52) have made careful studies of this factor and have found evidence of its presence with tests combining verbatim and substance learning. Henderson (37), King and Homan (45), and Welborn and Killian (74) incidentally found evidence of reminiscence in substance learning. McGeoch (52) found that the mean scores of the entire group may conceal reminiscence and that the percentage of subjects exhibiting reminiscence and the amount shown by such subjects are adequate and valid measures. Following this cue, English (22) reworked some of Killian's data. Forty-six per cent of the subjects showed gains over the immediate scores on verbatim recognition after 24 hours, while 52% showed gains on summary recognition. Thereafter the percentage declined for verbatims but increased for summaries or remained practically constant. After 71 days in one group the percentage of subjects with higher scores than immediately after the presentation was 54 for summaries, as compared with 22 for verbatims.

Retroactive Inhibition. The effect of interpolated tasks on substance memory is largely unexplored. However, McGeoch and

[1] This term, introduced by Ballard (7), while justifiable on etymological grounds, cuts across the more common English usages. For want, apparently, of any better term, however, it is rapidly gaining ground for either (a) the recall or recognition of any item which has once failed of reproduction without overt intervenient presentation or practice, or (b) a net gain in recall or recognition for a number of items due to the mixture of recalled and of forgotten items under such conditions.

McKinney (**53, 54**) made 2 studies of this kind. In the case of poetry, when part of the same poem and also nonsense syllables were used as interpolated material, small amounts of retroactive inhibition were found in both cases after 15 minutes. The susceptibility to inhibition was considerably greater after 7 days than after 15 minutes and was enhanced when recall after 15 minutes was omitted. A similar study of substance memory of a prose passage showed approximately the same results. The authors believe that "these experiments increase the generality of the decrement phenomenon and give support to the theory of inhibition as a major condition of learning."

V. The Technique of Investigation of Substance Memory

Control of Variables. We may well start with the assumption of Carr (**13**) that there are no constants and that variations in results are due to variations in conditions. The use of large groups of subjects is advantageous because the trend of results is not so much influenced by individual differences, daily fluctuations, etc., as it is in the case of small groups. The more important studies heretofore mentioned, including the earlier ones such as Henderson's, as well as many of the more recent ones, made use of large groups of subjects. The list of approximately 25 such studies will be omitted. In a few cases equated groups have been used. Greene's study (**34**) employed this procedure.

The problem of securing uniformity in difficulty of passages used in an experiment has been attacked in at least two ways aside from the use of the experimenter's judgment. Dietze and Jones (**19**) obtained the regression equations from data on their various pairs of passages and converted all scores into derived scores for one passage. Space does not permit the evaluation of their procedure. Jones and English (**42**) used the same passage for 2 kinds of testing, and English, Welborn, and Killian (**21**) based both kinds of test items used in their study on the same passage, thereby testing 2 kinds of memory for 1 learning task. The use of control groups for the purpose of eliminating the influence of prior learning is rather common, but the procedure of Dietze and Jones (**19**) for eliminating the effect of comprehension difficulties and poor habits of study, which has already been referred to, has been used by no one else so far as we know. The importance of the subject's knowledge as to whether and when he will be tested has already been mentioned; certainly this condition should be kept uniform in any

experiment. Practically all studies depend upon the subject's coöperation or motivation. The amount of exposure of the subject to the material has in some cases been regulated in terms of a time limit. Henderson (37) and several others used this method. The more common method and the one which seems to us to approach more nearly a true uniformity is that in terms of number of readings. Several of the most outstanding experiments, notably that of Anastasi (4), have employed only one reading.

Type and Length of Passage. There are enormous variations here, mostly unmeasured. The ease with which these factors are controlled—or seem to be—is, of course, a chief reason for their continued use. Travis (71) gives a useful methodological summary.

Test Technique. The method of reproduction has a certain logical justification in the case of substance memory and indeed it has been used in most of the pioneer studies as well as in some more recent ones. The usual scoring method is to divide up the passage into "ideas" and score one point for each "idea." There are two objections to this method. It precludes any investigation of reliability of measurement, and it is at the same time too subjective and not subjective enough. When the subject's reproduction varies in language or form from the original material, the court of last resort is the judgment of the experimenter. In some cases, *e.g.* Binet and Henri, substitutions have been rejected, but it is likely that in a majority of cases they have been accepted if approximately equivalent to the original. Henderson (37) found that the correlation between capacity for learning and for retention is shown better by scoring ideas than by scoring words. On the other hand Lodge and Jackson (50) and Travis (71) argue very strongly that scoring on "ideas" is not subjective enough. They contend that the method assigns too high a value to rote memory, that ideas should not be counted as equal in value, and that "judgment in organizing and condensing material, terseness and vigor of expression," etc., should be considered, whereas by the usual method the highest scores may be made by those who lack habits of selection and condensation. They prefer and use a qualitative scoring scheme based on a five-point ranking procedure as well as the usual scheme. Their data are too extensive for summary here. It seems to us that some combination of the 2 methods might be evolved through further experimentation.

Objective tests have been used very extensively in the more recent investigations, the commonest types being the recall or com-

pletion test and variations of recognition tests such as multiple choice and true-false. The first question to be considered is that of the reliability of measurements of substance memory by means of these tests. In recent years the practice of calculating reliability coefficients has become practically universal in all careful studies. We have at hand reliability data from a number of studies. An analysis of all of the data produced the following results.

(1) Reliability coefficients, 16 in number, obtained by correlation of chance halves or equivalent tests, range from .47 to .94, with a mean of .72. (In a number of cases the coefficient used is itself the mean of several in a whole study.)

(2) Repetition-of-test correlations were roughly grouped into short interval and long interval correlations. Those for short intervals, 8 in number, range from .47 to .94 with a mean of .79, while those for long intervals, 14 in number, range from .35 to .86, with a mean of .61.

The highest coefficients for multiple choice recognition so far reported seem to be those of Dietze and Jones (.90 to .94) and Anastasi (.861), and for recall that of Anastasi (.875). Outside of the field of substance memory, Strong (69) has shown that the reliability of recognition decreases rapidly with the lapse of time.

We can state, therefore, that in general the studies from which these correlations were obtained are sufficiently reliable, and we think that in the main the data for most of the important statements throughout the paper are reliable.

Two other important questions arise. Do recall and recognition tests measure the same functions? Is the recall type more difficult than the recognition type? Anastasi (4) has made one of the very few studies dealing with these issues in substance memory and probably the best study in the whole field. As to relative difficulty, her data show a negligible difference between the two types, and she says, "the alleged superiority of recognition over recall scores does not hold for logical memory . . . in which one is led to expect the greatest difference between the two processes according to previous writers" (p. 54).

As to whether recall and recognition are different processes, she obtained a coefficient of .23 in the case of 2 recognition tests with different material, but the coefficient for recall and recognition tests of the same material was .74. Also she reviewed other studies that attempt to establish differences between these 2 measuring procedures, particularly those of Achilles (2) and Lee (48), and found that their

data fail to show that recall and recognition are any more unrelated than recall tests for different kinds of material. Her final conclusion is that " material is more potent than method in determining inter-test correlations " (p. 54).

Unfortunately, control of the material used, as above noted, is peculiarly difficult in substance learning, though Bartlett (**8**) and Davis (**14**) contend that it is less so than with so-called nonsense materials. It has received so little attention that close comparisons between one study and another remain impossible. In regard to the number of repetitions, for example, there is for each type and length of material apparently a different optimum. Similarly for spacing, about all we can safely say is that for a given kind of material there is an optimal spacing; it is hazardous to set even approximate limits for this optimum. Indeed throughout the whole field one finds not only a lack of adequate data but even more a lack of incisive analysis. We are inclined to echo Snoddy's comment (**68**) that when to the simplest questions one receives a barrage of utterly contradictory answers, we can only conclude that we have not yet won through to a correct analysis and probably have not asked the right questions. Not a few have suggested that substance learning may have distinct laws from those governing nonsense syllables but only a few have devised experiments designed to discover those laws. The field is not new, but it is still wide open to fundamental research.

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THE CONTRIBUTION OF THE CHILD STUDY MOVEMENT TO CHILD PSYCHOLOGY

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The story of the child study movement, which ran its course between 1883 and 1917, is an interesting chapter in the history of child psychology. Although child study paralleled and at many points fused with the mental test movement, yet it is essentially a well-defined movement in itself. It caught the imagination and enthusiasm of parents and teachers as well as of educators, philosophers, and psychologists.

Immediately preceding had been a period characterized by theoretical discussions of the nature of childhood and a few observational studies of individual children written chiefly by philosophers and educational theorists. No one considering the preceding period could have foreseen the tremendous stimulation and growth of interest in the study of children that was to typify the movement. But despite this, the child study movement may be considered the climax of a trend of thought that had gradually been gaining impetus through the previous centuries. Only in America, however, did it have a widespread popular appeal. In Europe, particularly in Germany, child psychology continued to develop mainly along scientific lines. This paper is concerned with the popular movement as such.

The publication of G. Stanley Hall's study, *The Contents of Children's Minds on Entering School* (8), in 1883 introduces the movement, whereas the founding of the first institution for the scientific study of the child at the State University of Iowa in 1917 marks its close. It is true that several minor magazine articles had appeared before this. Among these were the studies by Holden (1877) (22) and Humphreys (1880) (23) on the vocabularies of children. These articles, however, excited very little attention.

The Beginning of the Movement

The spark which kindled the interest in the study of children into a conflagration was the influence of G. Stanley Hall. Hall was born at Ashfield, Massachusetts, on February 1, 1844, of Puritan ancestry.

He graduated from Williams College in 1867, studied in Germany two years, and graduated from Union Theological Seminary in 1871. As time passed he found himself lessening in sympathy with current orthodoxy. After holding first the chair of English literature, then one in modern languages, and finally, one in philosophy at Antioch College from 1872 to 1876, he studied under William James at Harvard and received the doctorate degree there in 1878. Then for two years he pursued further studies in Berlin, Bonn, Heidelberg, and Leipzig, working with Wundt, Helmholtz, and other noted investigators. From 1881 to 1888 Hall was professor of psychology at Johns Hopkins University where his laboratory was the first of its kind in the country. In 1889 Hall was installed as first president and made professor of psychology of the newly formed Clark University at Worcester, Massachusetts. He founded the *Pedagogical Seminary* in 1891; much of his work in child psychology appears in that journal. His studies of children began as early as 1880 and the first objective evidence of them was published in *The Contents of Children's Minds on Entering School* (**8**). This study aroused an interest in a psychology of a more experimental, measurable type than the then current descriptive psychology.

His report was worked out on the basis of a questionnaire which aimed to inventory the contents of the minds of children who were entering the primary schools of Boston. The children were questioned in groups of three by a teacher concerning their knowledge of animals, growing plants, parts of the body, natural geography, geometrical figures, number concepts, colors, occupations (such as those of shoemaker, bricklayer, etc.), and miscellaneous subjects that were taken for granted in the schools as being familiar to entering children. As a result of this investigation, Hall concluded that there was scarcely anything of pedagogic value whose knowledge might be assumed at the beginning of school life.

Immediately Hall and his students designed and used questionnaires of a similar sort. These were circulated to parents and teachers throughout the country. Some of the topics of the questionnaire studies follow:

The sense of humor among children
Children's appetites and food
The sense of self
Children's collections
Reactions to light and darkness
Tickling, laughing, and the comic
Stillness, solitude, and restlessness

Fears
Punishment
Dreams
Memory
Anger
Envy and jealousy
Dolls
Perception of rhythm
Motor ability
Toys and playthings
Bashfulness
Cloud and moon fancies of children
Moral and religious experiences of children

In interpreting the data collected by means of these questionnaires, Hall used only the simplest methods, confining his statistical treatment to a few percentages.

Typical of the studies using the questionnaire method is *A Study of Fears* (9) by Hall. The syllabus was filled out by 1,701 people, mostly under twenty-three years of age "gathered in different places and methods without great uniformity, and 386 supplementary reports and many returns or special points, all written on nearly 4,000 pages" (p. 151). Thus Hall's study had the merit of extensiveness even though it failed to meet present day standards of scientific reliability and validity.

The following passages which were preceded by a listing of six descriptive categories of fears, such as fear of celestial phenomena (winds, storms, etc.), special inanimate objects (fire, water, drowning), living things (dogs, cats, snakes), fears of disease or dying, fears of supernatural, and fears induced by the suddenness of an experience, are quoted from the syllabus.

"7. In each case state order and age of fears, how long they lasted, how intense they were, what acts they prompted, and educational good or bad effects; was sleep affected? . . .

"The syllabus is drawn up . . . and sent to you with the request that you will read it carefully item by item, and (1) jot down at once in the easiest form of notes, whatever each paragraph or phrase recalls of your own childish fears; (2) that if you are a parent you will add to this any observations this paper may suggest or recall on your own children . . .; (3) that if you are a teacher, you will read this paper to your class, write it on the board, or give it to individual pupils (of upper grammar or high school grades) and ask them to write as an exercise in composition (setting apart an hour, or asking for out of school work) an account of their own early or present fears; (4) if you are a normal school principal or teacher of psychology, you may connect it with the class work in the study of feelings or emotions; (5) if you are a

principal or superintendent, you can assign the work to some teacher or advanced pupil to collect the data . . ." (p. 149).

Even a cursory glance at the questionnaire informs one that Hall would obtain by its use the results that he did obtain. Modern investigators have shown that a great deal depends on the way that a question is stated. Many of Hall's questions suggest their answer. This fact alone, undoubtedly, invalidates many of his results. From the historical point of view, however, they are of interest still because many of them are not so very different from those of more recent and reliable studies. The 1,701 persons described 6,456 fears which were roughly grouped as follows according to objects feared.

Fear	Incidence
Celestial Phenomena	
Thunder and lightning	603
High wind	143
Cyclones	67
Clouds and their forms	44
Meteors	34
Northern lights	25
Comets	18
Fog	16
Storms	14
Eclipses	14
Extreme hot weather	10
Extreme cold weather	8
Total	996
Darkness	432
Ghosts	203
Dreams	109
Solitude	55
Total	799

Fear	Incidence
Animals	
Reptiles	483
Domestic animals	268
Wild animals	206
Insects	203
Rats and mice	196
Cats and dogs	79
Birds	51
Total	1,486
Fire	365
Water	205
Drowning	57
Total	627
Strange persons	436
Robbers	153
Total	589
Death	299
Disease	241
Total	540
Miscellaneous fears	1,419

Next the fears of 500 boys (1,106 well-described fears) and the fears of 500 girls (1,765) were tabulated to show the influence of sex.

Of the 500 girls 230 reported fear of thunder and lightning, while the same number of boys reported this fear but 155 times. In fears of the end of the world, rats and mice, blood, and being buried alive, girls excelled boys whereas boys excelled girls in fears of water, height, and shyness. Each of the boys had 2.21 fears, the girls, 3.55 fears. It was also found that the fears of boys increased from seven to fifteen and then declined; the fears of the girls increased more steadily from four to eighteen. Thirty-six boys below four

years of age had 1.76 fears each whereas seventy-four girls of these age levels averaged 4.39 fears each.

Although Hall's study may be criticized in terms of the reliability and validity of the questionnaire used, the conclusions he derived from these data have a distinctly modern note. Hall concluded that there is no one without fears, that fears are necessary and healthy, and that many fears are the result of heredity because the experience of the individual and even that of the near forbears cannot explain all fears. (The influence of the recapitulation theory is evident here.) He also states that "While many special fears decline and others increase with age, many infantile fears remain through life . . ." (p. 154). From these statements it is clear that Hall was inclined to generalize beyond the limits of his data. To prove the hereditary nature of certain fears would require an elaborate and extensive series of investigations meeting scientific criteria little realized or appreciated until recent times.

It is apparent even to the casual reader that child study as conceived by Hall included not only the psychological aspect but many other phases of the development of the child. A definition of child study given by Hall (11) follows:

"It is a nondescript and, in some sense, an unparalleled movement—partly psychology, partly anthropology, partly medico-hygiene. It is closely related at every step to the study of instinct in animals, and to the rites and beliefs of primitive people; and it has a distinct ethico-philosophical aspect—partly what a recent writer classed as the higher biology—with a spice of folk-lore and of religious evolution, sometimes with an alloy of gossip and nursery tradition, but possessing a broad, practical side in the pedagogy of all stages. It has all the advantages and the less grave disadvantages of its many-sidedness" (p. 689).

In 1895–1896 Hall issued a *Topical Outline for Educational Studies* (19). Typical of the questions raised regarding the value of the Froebelian gifts in the kindergarten are the following:

1. How much time is taken up in giving out and putting away each kind of material, do the children themselves do it, reasons pro and con, and how many minutes are occupied in actual work on each?
2. Which of Froebel's gifts and occupations are most used and most enjoyed?
3. How often and when do you see slight tremors of hand or flushing face?
4. What other material than Froebel's is used, in what proportion, with what success, and how liked?
5. Does a fixed regime, as of gifts and occupations, allow us to find out or to develop the natural aptitudes and interest of children?

On the basis of these questions, Hall (**10**) criticized the kindergarten because it usually offered an unhygienic physical environment for the child, provided very little outdoor life, overstimulated the child mentally, offered little opportunity for nature study, and centered too much of the child's activities around the gifts. He felt that there were hundreds of things quite as suitable for the child as these conventional gifts and occupations—the whole toy world should be available. Furthermore there should be plenty of opportunities for free play with these toys. Later (in 1898) Hall's criticisms were taken up at the International Kindergarten Union, and eventually resulted in a widespread correction of these defects in practices. Thus to the child study movement with all its weaknesses and limitations can be credited an influence on the reformation of kindergarten practice.

Preyer's *Die Seele des Kindes* (**34**), although properly belonging in a consideration of the development of child psychology along scientific lines, was admired by G. Stanley Hall who was responsible for the American translation (**35**). This in turn was the motivation for Shinn's *Notes on the Development of a Child* (**39, 40**), a classic in child study literature and the example for a long series of observational studies in America.

In observing her niece Shinn did not use any special form of blank but merely took copious and detailed notes from day to day. She used Preyer's notes a great deal; in fact she admits that for some months her notes were largely memoranda of likenesses and differences between her niece and Preyer's son. It is obvious that in using such a method an observer sees largely what she is looking for. This is perhaps the severest criticism of the early observational studies. Observational studies of the present time differ sharply from the older studies in this respect. Furthermore, the modern observational study defines exactly what is to be included in the observations and excluded from them. Not only are the items to be observed defined, but the reliability of the observer is determined—two individuals observe simultaneously and independently, then compare results. It is apparent that the older studies discussed in this article used no such safeguards and as a consequence lacked both scientific validity and reliability. Even though the results of these early observational investigations cannot be accepted now as scientific, the impetus they gave to child psychology should not be minimized. They are of interest as first steps to more rigorous and scientific endeavors.

Among the observational studies of children following Shinn were ones by Blanton (3), Dearborn (6), Mrs. W. S. Hall (20), Hogan (21), Major (28), McLeish (29), Moore (30), Munro (31), Prior (36), and Simpson (42). Many such observational studies were made by persons with little or no scientific background—enthusiastic parents or maiden aunts of the child—with the result that such studies soon fell into ill-repute. What started out to be a serious scientific endeavor ended by being the pastime of mothers' clubs and teachers' organizations. Many serious workers saw the danger to the movement in such projects but too late—the damage had been done, criticism was aroused, and the child study movement waned.

Much of the outcome of Hall's work with the questionnaire was shown in a series of books which Hall published during the period from 1904 to 1911—the large two-volume work *Adolescence* (12), the abridgment of this in *Youth* (18), *Aspects of Child Life and Education* (16), and *Educational Problems* (14), another two-volume work.

Hall's psychology is based on a major premise. This fundamental principle which he calls a " general psychonomic law " is an adaptation to psychology of the concepts of biological evolution and the doctrine of physical recapitulation. It assumes that the psychical life and its expression in the individual develops from birth onward through a series of stages more or less clearly corresponding to those through which early man and his ancestors are supposed to have passed. According to Hall the normal growth of the mind necessitates the passing through of each stage, because the development of any one level is the normal stimulus to the next. This principle is the guiding thread of his whole theory of child development. Thus his child psychology is a kind of nature study of children's minds rather than a psychology in the scientific meaning of the term.

In addition Hall works out more or less detailed descriptions of five stages of life, with suggestions regarding the racial period which formed the stages and with the outlining of the type of education desirable for each one. Each stage is different from that which precedes and follows. The four main periods or developmental stages are: (1) infancy, (2) childhood, (3) youth, and (4) adolescence. The period of infancy and early childhood harks back to remote animal ancestors. Infancy is a period of physiological development and sensory experience; childhood (the period from two to eight), in comparison with the years preceding, is marked by slow increase in height and weight. These years are a time of free activity, naturally devoted almost entirely to play. A third period in human life is the age from eight to twelve, youth. The brain is nearly adult in size and weight, health is almost at its best, activity is greater and more varied than at any other time, and there are peculiar endurance and resistance to fatigue. The child now acquires a life of his own outside of the family.

Adolescence is a time when the emotions dominate. Development is more saltatory, suggestive of some ancient period of storm and stress when old moorings were broken and a higher level was attained. In addition to the physical changes, the changes that occur in the mental life are so many and so radical that it is difficult to describe them all.

> There are increased morbidity and diminished morality, progressive assertion of sex, and many other changes. The child resents authority and dislikes routines. Then comes later adolescence, about the nineteenth year, marked in boys by a practical cessation of growth, by some decline in the curve of health, and by "other phenomena that suggest that, having achieved adult size, there is likely to be a period of slightly reduced vigor as if to rest and adjust after being cast up by a flood-tide on the shore of manhood a little exhausted. . . . At this point perhaps the individual represents the phyletic stage where, in its survival value in the struggle for existence, the advantages of increased size and strength began to be surpassed by those of a higher mentality and the main current of evolution swerved thought-ward" (12, pp. 42–43).

Hall considered play as one of the most important problems of child development. Play is not the mere excess of motor activity; it involves all the interests of the child and is the center of his whole existence. In his plays and games the child repeats the history of the race.

According to Hall, fear is a primitive instinct or emotion. It is more far-reaching in its effects and more manifold in its expression than can be discovered by the study of any one child. It is only by studying childhood, by studying the fears of many individuals, and by taking note of its physical manifestations in animals that an adequate concept of fear can be formulated. Furthermore, there is no rational explanation of fears that does not take into account racial experience. This is evidenced by the fact that most of our fears are not practical; they do not protect us against our present dangers but are adjusted to an older order. We do not fear that which in our protected environment is most dangerous to life. Indeed our fear reactions are in many cases the wrong reaction in modern life although under primitive conditions they undoubtedly had survival value.

It is doubtful whether any single man up to the present time has done so much to foster interest in child psychology as G. Stanley Hall. His enthusiasm for and insight into the possibilities of research with children made him for the time the unquestioned leader of the child study movement. Moreover, his contributions cannot be measured

merely in terms of his own work, since few men have been able to impart their own enthusiasm to so many other individuals.

Among the 81 doctorates conferred by Hall during the period of his presidency of Clark University, the following people may be noted: Terman, Gesell, Goddard, Mateer, Blanchard, Starbuck, and Kuhlmann; all of these persons are active contributors in the field of child psychology at the present time. His published material consisted of approximately 14 books and 350 articles, short papers, and addresses (**45**). His gradual shift away from the laboratory toward genetic psychology and the larger social and religious questions is revealed by a list of his contributions.

The influence of Hall's work was certainly widespread. Following immediately on the heels of his studies there developed a great diversity of effort along child study lines. Societies and associations for child study were formed in this country: one by Hall in 1893 (The National Association for the Study of Children); one in Iowa and Illinois in 1894; one in Nebraska in 1895; one in Kansas in 1896, and so on. For several years the Illinois Society for Child Study published its transactions.

Reverberations of Hall's work were felt in Europe. A child study association was formed in England in 1894, at the instigation of certain British teachers who attended the 1893 International Congress of Education at Chicago at which Hall was one of the speakers. Societies were founded in Poland in 1897, Germany in 1899, and France in 1901. The first International Congress for Child Study was held October 1 to 6, 1906, at Berlin and was attended by nearly 700 people from Germany and other countries. To facilitate the work, the congress was divided into four parallel sections: the anthropological, the psychological, the psychological-pedagogical, and the philanthropic-social.

In addition a number of periodicals were established in connection with the movement: In America, the *Pedagogical Seminary* in 1891; the *Child Study Monthly*, 1895; in England, the *Paedologist* (the organ of the British Child Study Association), 1899; in Germany, *Die Kinderfehler*, 1896; *Die Kinderseele*, 1900; and in France, *Bulletin de la societé libre pour l'étude psychologique de l'enfant*, in 1901.

All of this work should logically have resulted in an institute for the scientific study of children. Indeed in 1909 Hall (**13**, **17**, pp. 401–402) attempted to found such an organization but sufficient funds were not available.

The departments of the institute as planned by Hall were as follows: (1) a collection of literature, including the hundreds of educational bills passed every year; (2) natality, birth and death rates; (3) hygiene (mental and physical health); (4) work with subnormal children; (5) crime (delinquent children); (6) vice (sexual aberrations); (7) child linguistics; (8) anthropological and sociological studies; (9) experimental didactics (experimental investigations

of teaching methods); (10) child labor; (11) moral and religious training; and (12) pedagogical museum. Six of the above departments were partially organized. The aim, according to Hall, was to bring child philanthropy and field workers in contact with scientific paidology. However, it was not until eight years later that such an institution for the study of children was established in Iowa and when it did come it came as the result of the inspiration of an Iowa woman rather than as the direct culmination of the child study movement.

When the work of Freud and Jung in psychoanalysis became known in America, Hall took up with the new doctrines with his customary zeal. His emphasis upon feeling and his centralization of adolescence about sexual development and the evolution of love made him an eager student of the Freudian views. However, he never fully accepted the Freudian doctrines of the all-dominance of sex because he regarded hunger of equal importance. Moreover, psychoanalysis fitted in with his oft-expressed belief in the inadequacy of introspection as the sole method of psychology and with many of his statements savoring of behavioristic psychology. Still later Hall revived an old interest in the psychology of religion and then turned from this interest to the psychology of senescence. Thus, his life was a long series of enthusiasms.

Other Contributors to the Movement

In *Mental Development in the Child and the Race* (1), James Mark Baldwin tried to do for the development of the child what Darwin undertook for the animal series in his *Origin of Species.*

In this book he applies the principles of evolution to mental development, and attempts to show that what holds good for organic life is true also for mental life. After the first three or four chapters very few observations and experiments made upon his own or on other children are given; the book is devoted almost entirely to theoretical considerations. For this reason Hall and his co-workers looked askance at it.

The appearance of Thorndike's *Educational Psychology* (47) created something of a stir among educationists and psychologists, particularly psychologists interested in the study of children, for it promised to make the treatment of educational problems far more precise and definite.

It was proposed to apply to all such problems the methods of exact science. Accurate quantitative measurement was to be substituted for the more or less skillful guesswork which was current in educational writings of the time. Theoretically it would be possible by the use of these methods to develop a genuine science of education, whose propositions would have as high a degree of validity and reliability, perhaps, as botany or zoölogy. In short, it was

proposed to measure mental traits and changes as affected by educational processes, instead of simply estimating them. To do this he believed many children had to be studied experimentally. That there was some opposition to this point of view is indicated by an article of O'Shea's entitled *Tendencies in Child and Educational Psychology* (33). O'Shea stated the belief that even though such authorities as Thorndike advocated experiments upon children *en masse* such experiments could not be of great service unless there were a great number of individual records or life histories made by skilled observers who followed a child or group of children for several years. He believed that cross-sections of development in respect to any trait had little meaning except when the whole developmental process was available for study. There was no significance, for education at any rate, in an isolated fact of child-nature; what preceded it in the causal series, and what it led into must first be known.

The aim of King's book, *The Psychology of Child Development* (24), is "to present a consistent and intelligible outline of the mental development of the child from the standpoint of mental function" (p. v).

Professor John Dewey in the introduction states "the true value, scientific and practical, of child-psychology is not that we may know this or that fact about children, . . . but that we may know how the growth of a human being proceeds, what helps and what hinders, what furthers and what arrests it, and how these results are brought about" (p. xvii). King rejects all the usual modes of infant observation inaugurated by Preyer, and holds that neither the time nor order of appearance of activities of the various senses or emotional attitudes is of any importance and, if it were, would be impossible to observe because the first manifestations of mental processes are entirely undifferentiated. The entire child is essentially in every reaction and it is only to the observer that he seems now in a state of emotion, now in one of cognition. For King, the problem is not when nor in what order experiences occur but "How does experience differentiate into volition and cognition and under what circumstances does the emotional attitude stand out in experience; and what must such an attitude mean in the undeveloped consciousness?" (p. 19).

The general principle considered as underlying the child's mental life by Tracy in his *Psychology of Childhood* (48) is the so-called principle of transformation. Briefly this principle states that every mental phenomenon passes through a graduated ascending series of development.

At first, the physiological dominates, consciousness is at a minimum; the mental phenomena are limited to the reaction of the nervous system to external stimuli or to organic conditions. "For example, the child cries at intervals from the moment of his birth, but at first this cry is independent of his will and possesses scarcely any mental significance, for it is made without cerebral coöperation" (p. xiii). Later the mental aspect becomes more prominent. When the intellect and will have become sufficiently developed, the child directs his attention to the act and performs it voluntarily. The process, when viewed

from the outside, has not changed at all, but when viewed from the inner side "it is seen to have been completely transformed in character." One of the psychologist's most difficult tasks is to determine the when and how of this transformation. However, the exact time at which each psychic activity makes its appearance is of less importance than the order of various activities.

Other individuals producing texts dealing with child study include Chamberlain (5), Kirkpatrick (25), Luckey (27), Tanner (44), and Taylor (45).

In 1916 Watson (49, 50) began his classic work on the basic emotions. He reported that fear was obtained in infants by removal of support, and a sudden push or pull. The love response was elicited by stroking, patting, shaking, and rocking. Rage was shown when movements were hampered by holding the nose or arms. Because recent experimenters have largely disproved Watson's findings, it is probable that Watson's major contribution lies not so much in the specific conclusions about the few responses, as in the great stimulus he gave to the development of experimental work in all aspects of child psychology. The growth of the experimental approach to the study of children is another story. Watson's work forecasts a new day in the study of children but is not a part of the child study movement as such. It did hasten the end of the waning movement because it turned the attention of those with a scientific interest in the study of children to the possibilities of genetic research.

Criticisms of the Child Study Movement

The faults of the feverish and artificial activity in the use of the questionnaire and observational method were soon apparent and a storm of criticism was directed at Hall and his students in particular and at the child study movement in general. Professor Hugo Münsterberg began the attack in an article in *The Educational Review* (32).

He bemoaned the fact that "the good appetite of psychology has become in our day something of voracity and she has begun to devour all mental science, history, social life, ethics and logic, and finally metaphysics." The limits of psychology, he believes, are easily understood. Psychology considers the mental life as an object which must be analyzed and explained—analyzed into elements and explained by laws. The psychologist, therefore, accepts as presuppositions that the mental life is such an object and these objects are combinations of elements controlled in their connection by causal laws. He states, however, that:

". . . In the reality of our inner experience our mental life has not all these characteristics: the ideas are objects, while the feelings and volitions are

subjective activities, and these objects are experienced as wholes and units, not as composites, and these activities are controlled by freedom, not by laws. Psychology thus presupposes for its purposes a most complicated transformation of the reality, and any attitude toward the mental life which does not need or choose this special transformation may be anything else, but it is not psychology . . ." (pp. 112–113).

Furthermore Münsterberg believed that as soon as this elementary philosophical principle is clear it is no longer possible to doubt that "most of the so-called child psychology is partly history, partly economics and ethics, partly physiology, partly nothing at all, but decidedly not psychology" (p. 114).

However, he is willing to admit that if everything is excluded from child psychology which is not really psychological, there remains still a good set of problems which belongs strictly to the psychology of the child:

". . . the analytic study of its perceptions and associations, its memory and attention, its feelings and emotions, its instincts and volitions, its apperceptions and judgments to be described and explained with regard to their elements and laws; but this group can certainly not be separated from the psychology of the adult. . . ."

Thus child study if it is to be of service to general psychology must first of all select its problems.

However, even if child study were to do this Münsterberg could see little use in it. He asked:

". . . What is the use of analyzing with the doubtful means of indirect observation those psychical states which we can find as the objects of direct observation in our own minds? . . . It follows, secondly, that the work must be done by trained specialists or not at all. That child study which has for its aim only the collection of curiosities about the child, as an end in itself, may be grateful to the nurse who writes down some of the baby's naughty answers or to the teacher who sacrifices half an hour of her lesson to make experiments in the classroom to fill out the blanks that are mailed to her. . . . And it is not only the lack of technical training which brings these contributions so near the hunting stories and their value for scientific biology. No, it is, above all, the absence of the psychological attitude" (pp. 114–115).

Although child psychologists of the present time would undoubtedly agree with Münsterberg on the second point, they would deny his position on the first strenuously. Clearly it is the old and often repeated objection to the scientific study of children in terms of the child's inability to introspect.

His final point of objection was that even if psychology were willing to accept "the stuff" as reliable and truly psychological, as

long as the thousands of "little facts" are not connected by a theory, the facts are dead masses and of no consequence.

As was to be expected, other writers were defending the movement (51):

"To those who say child study is a fad, the answer is no less ready and overwhelming. There are many crudities of method and trivialities in the way of results. Many people are attempting kinds of work in its various fields for which they are totally incompetent. Only those trained, as I have said, in the study of insanity, anthropology, philosophy, and laboratory psychology, etc., and who can wield all the resources of these great departments, are fully trained for this work, but every one can help not only himself, but the expert. The dimensions and force of this movement are shown by the fact that, like other great culture movements, it has overflowed the traditional channels of academic work, and has become a popular movement, which, like the Reformation that it consummates, has as its watchword *Individuality*. Few have yet seen the vast scope of the movement. One of its best features is that it is a new point of contact between the people and scholars, and is bringing together the university and kindergarten as never before in history" (p. 211).

The criticism of J. Mark Baldwin (2) is typical of the early comments directed at the questionnaire method.

". . . In the opinion of the present writer, results obtained by the syllabus method have very little value. They lack the first requisites of exact method; and moreover they are often further vitiated by a certain speculative philistinism and crudity of result. There is absolutely no way to control the reports. . . . Child study is a fad, a harmless one for the most part; indeed, a beneficial one to those teachers who lacked humanity before and are now finding it in their attitude toward their pupils. But it is an insult to the teaching profession to tell them that their humanity needs this sort of cultivation, and to hoodwink them into thinking that they are making contributions to science."

The Founding of the Iowa Child Welfare Research Station [1]

The founding of the Iowa Child Welfare Research Station in 1917 marks the close of the child study movement as here we find for the first time an emphasis laid on the scientific value of the study of children rather than upon its educational value. Its founding heralded the dawn of a new day in the study of childhood. Childhood had come into its own as a problem of major scientific interest to be

[1] A detailed account of the founding of the Station may be found in the first two parts of *Pioneering in Child Welfare* which were prepared by the writer from original documents and files of the Hillis family and the University of Iowa. [Bradbury, D. E., and Stoddard, G. D.: *Pioneering in Child Welfare: A History of the Iowa Child Welfare Research Station 1917–1933.* Iowa City, Iowa: State University of Iowa. 1933. Pp. 80 (pp. 1–69)].

studied not by the parent, the teacher, the philosopher, or the educator, but by the scientist.

An Iowa woman was responsible for the founding of the Station. This woman was Mrs. Cora Bussey Hillis of Des Moines. Mrs. Hillis, faced with the rearing of her own children, recognized the need for the scientific study of childhood. She initiated and directed a campaign that resulted in the act establishing the Station, April 21, 1917. In June, 1917, President Jessup appointed the first advisory board of the Station representing various departments interested in the general field of child welfare, under the chairmanship of Dean Carl E. Seashore. In August the directorship was offered to Dr. Bird T. Baldwin and the Station became a functioning unit of the University with his arrival in September, 1917.

Evaluation of the Child Study Movement

What of the child study movement? What, if anything, did it contribute to child psychology? First of all let us see how it was evaluated by G. Stanley Hall, himself. In *Aspects of Child Life and Education* (16) written at a time when child study had waned (1921) Hall states:

> "The wave of interest in child study which swept over this country some three decades ago, and even inundated Europe, was a culture movement of great importance, no matter what value we attribute to its scientific results. It taught us that the child and its characteristic traits are ages older than adulthood, which is a comparatively recent superstructure, and that success in life is far more dependent than we had realized on a happy childhood. Another effect of the movement was to give psychology, which has been slower and more reluctant than even religion to recognize evolution, something of a genetic trend, which has been greatly reinforced of late, at least for a large and important group of scientific minds, by the new conceptions of childhood contributed by the psychoanalysts . . . the value of not only the school but every institution is how much it contributes to bring individuality to the fullest and most mature development of which it is capable" (pp. v–vi).

Child study mainly made use of two methods—the observational and the questionnaire method. At the peak of its popularity, enthusiastic teachers and parents announced with the utmost finality the results of their hasty excursions into the field of mental development. Immediate generalizations were easy to obtain with the use of the "syllabus" method. With this system a few questions on some subject concerning the child were formulated, sent to a few score, hundred, or thousand teachers or parents depending on the enthusiasm of the questioner, returns were tabulated, and discoveries proclaimed. On the surface, such studies seemed simple and conclusive until the

weaknesses of the method gradually became apparent. Other equally enthusiastic followers armed with pencil and paper proceeded to record all that Johnny or Mary was observed to do and then made hasty generalizations about children. Too often in evaluating the movement we see only this feverish activity and ignore what really was accomplished by the handful of serious workers who were making an honest attempt to study children objectively. Toward the close of the movement when the weaknesses were realized, the questionnaire and observational methods were revamped, and new methods formulated.

The net result in terms of actual findings was not important for either psychology or education. In fact the number of thorough observational studies seems surprisingly small in view of the large number of publications appearing during the period. Not more than a dozen children were studied thoroughly; furthermore the lasting results were comparatively few. The questionnaire method yielded little more; Hall's monumental work on *Adolescence* is undoubtedly its most important contribution.

But in spite of this the child study movement made a real contribution to the study of the psychology of childhood. It resulted in (1) an increased recognition of the importance of an empirical study of child psychology and of educational problems in general, (2) a realization of the necessity for a critical evaluation of method, (3) a comprehension of the importance of childhood *per se*. For the first time the child became the center of psychological and educational thought.

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A DISCUSSION OF RUCKMICK'S CRITICAL REVIEW OF AUDITION

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In a current number of this journal [1], Ruckmick reviews recent activities in the field of audition, and in that connection discusses our experiments and the interpretation of our results. With many of his statements and criticisms we are not in agreement.

1. After a description of our studies of electrical potentials from the auditory nerve, Ruckmick says: "The data indicated that the basilar membrane could not be selective in an analytical way, but that the total frequencies were being transmitted through the auditory nerve to the brain" (p. 418). This conclusion is imputed to us, and we are criticized for forming it.

We have never at any time taken the position which the quoted statement describes, and we do not now hold any such position. The argument behind the statement is plainly a *non sequitur*; for the presence of frequencies in the auditory nerve as a whole does not tell whether analysis has occurred in the cochlea or not. We have never said that our experimental results were opposed to the principle of peripheral analysis by resonance or otherwise. On the contrary, we have said repeatedly and emphatically that both resonance and non-resonance theories are still possible in the light of our results. On the basis of other results, moreover, we have declared ourselves in favor of the peripheral analysis principle, and have embodied it in the resonance-volley theory. Consequently, Ruckmick's comment, in the same paragraph—"How contradictory these results are in terms of the original claims by Wever and Bray may be gathered from reading their earlier publications"—makes us wonder whether he has indeed read these or later publications on the subject, for it should be clear that the "claims" are not ours and the "contradictions" not of our making.

2. In the paragraph following that from which the preceding quotations are taken, Ruckmick says: "In the face of such results

[1] C. A. Ruckmick, A Critical Review of the Field of Audition. *Psychol. Bull.*, 1936, **33**, 407–431.

Fletcher reviewed the two types of theory . . ." and ". . . proposed a combination of these theories . . ."; and in the next paragraph goes on, "Wever has recently [referring to a 1933 paper] lent his support to such a compromise. . . ."

The paper of Fletcher's which is referred to was read before the Acoustical Society of America in December, 1929, and could hardly have been prepared "in the face of" the results of Wever and Bray, which then had not even been obtained (they were announced in February, 1930). Naturally, Fletcher's article, like his book, *Speech and Hearing,* of 1929, which also contained his theoretical views, bore no reference to our auditory nerve studies. Indeed, it is particularly to Fletcher's credit that he worked out the combination, space-time theory in advance of any direct information on the action of the auditory nerve. Our support was lent to a combined theory, not first in 1933 as Ruckmick makes out, but in 1930, almost simultaneously with the presentation of our results on auditory nerve impulses.[2] And while we have sought not to be dogmatic in our position, we have continued to support this type of formulation.

The difficulty and misunderstanding regarding our views seem to have come from two sources; the traditional opposition between the "place" and "telephone" theories, and our own statements that the Helmholtz theory required revision in the light of our results.

When evidence appeared that the frequency relation hypothesized by the telephone theories is true (within limits), it was easy to assume that the rival, place hypothesis was wrong; and of course it was particularly easy to suppose that those who brought forth this evidence must share the assumption. But tradition and traditional rivalries are no safe guides to thinking; the place and telephone principles are not incompatible, as Rutherford was acute enough to perceive as long ago as 1898. And as Fletcher, Troland, and others, including ourselves, have demonstrated, these two principles can coexist in a reasonable, consistent theory.

Our caution that the Helmholtz theory, in its present form (or forms), must be revised or else be discarded in favor of some other formulation should not have led anyone to the conclusion that we proposed to abandon all features of that theory, and the theory itself in every possible version. There is danger of misunderstanding here, for the Helmholtz theory in modern dress is not very like the theory that Helmholtz proposed. The danger is particularly serious because

[2] E. G. Wever and C. W. Bray, Present Possibilities for Auditory Theory. *Psychol. Rev.*, 1930, 37, 365–380.

the modern formulations very often are not theories of hearing at all, but theories of pitch, and it is not until care is taken to work out a theory which includes intensity and other essential phenomena of hearing that the differences from the original Helmholtz theory are made obvious. Speculations regarding "place," resonance, and analysis are not enough; some assumption must be made, for example, regarding the frequency of impulses in the auditory nerve. The suppositions regarding frequency of auditory nerve impulses that were embodied or implied in the Helmholtz theories current in 1930 were incompatible with our results, and we pointed out the fact. Also, we showed in our formulation of the resonance-volley theory at least one of the lines of revision which the Helmholtz theories might take.

3. Repeatedly in his review, Ruckmick belabors the point that "electrical acoustical manifestation by no means indicates mental auditory sensation," and inveighs against "the unguarded assumption that physiological determinants are psychological data" (p. 410). Ruckmick's discussion has the force of a general criticism of workers who have used electrical and other physiological methods, for he fails to mention that these workers themselves have been chary of "unguarded assumptions" and cautious in the interpretation of their results. And as Ruckmick has not done, they have gone to the trouble to examine the evidence which bears on the relation between "electrical acoustical manifestation" and "mental auditory sensation."

In our auditory studies, we have been aware of the distinction between physiological and psychological data. For example, we have taken pains to emphasize, in a discussion of results on the frequency ranges of the auditory nerve responses, that "the results . . . do not of course give any evidence on the extent to which these frequencies pass the higher synapses and enter into the complex response patterns of the animals."[3] We have been interested, all along, in comparing the results obtained by the electrical methods with results obtained by behavioral methods, and have gone to the trouble to gather behavioral data for the purpose of this comparison. It is hardly fair to represent the matter as though persons using the electrical methods have proceeded in the belief that they were directly dealing with "mental auditory sensation."

4. In discussing methodology, Ruckmick says: "Then there is

[3] E. G. Wever, Impulses from the Acoustic Nerve of the Guinea Pig, Rabbit, and Rat. *Amer. J. Psychol.*, 1931, **43**, 460.

the Wever-Bray electrical response technique which is still undergoing appraisal as to just what it signifies. Several investigators deny that this electrical response is of the order of an action current, and Wever and Bray no longer call it by that name" (p. 422). Now the important thing is the significance of the data that the technique brings to hand, and these data vary according to the tissues on which the electrodes are placed (and the precautions that are taken in the process). Given suitable precautions, a nerve will yield nervous impulses, and sensory cells will yield sensory impulses. When Ruckmick says that "Wever and Bray no longer call it by that name" he is evidently confused by the fact that the responses of the auditory system may be studied at various levels: in the cochlea, in the auditory nerve, and in various tracts of the central nervous system.[4] When we have dealt with nerve impulses we have called them such, and when we have dealt with effects from the cochlea we have used another name. The fact that it is technically difficult to achieve a perfect isolation of the effects at different levels should not obscure the distinction.

5. In his discussion of stimulation deafness experiments, Ruckmick stigmatizes Horton's work as representing "a curious reversal" of logic, and criticizes it on the ground that "tones of such great intensity can be followed only by a pathological function and ultimately by a structural change from which few, if any, deductions can be made as to the normal state of affairs" (pp. 412–413). Naturally, the study of stimulation deafness is a study of pathological function; if there is no pathological change the experiment is a failure. Tones of great intensity are used in such experiments because weak tones do not produce stimulation deafness. The point that Ruckmick seems to have missed is that Horton was repeating, with modern, precise techniques, a type of experiment that has many times before been done by poorer methods. Horton was properly aware of the dangers of theoretical speculation on the basis of the results of such experiments, and was cautious in his statements in the papers mentioned by Ruckmick. And a later paper of this series, not mentioned by Ruckmick, bears this statement, which was made deliberately to forestall rash inferences: "It is probable that when a tone attains an intensity of 100 *db.* or more, it involves practically the entire extent of the basilar membrane and allied structures. It should be added however, that *these results do not indicate a cor-*

[4] See, *e.g.* H. Davis, The Electrical Phenomena of the Cochlea and the Auditory Nerve. *J. Acoust. Soc.*, 1935, **6**, 205–215.

respondingly great spread for tones of more moderate intensity[5] (italics ours).

6. Ruckmick, in his review, outlines a most surprising theoretical position: a place theory for low tones, coupled with a frequency theory for tones above 8000~ (pp. 419–420, 428). Here is a "curious reversal" indeed; for heretofore the proponents of frequency theories have worried most about the nervous representation of high tones, and the advocates of place theories have been most concerned with the spacial differentiation of low tones.

7. Ruckmick's argument for a frequency representation of the highest tones is based indirectly upon Culler's evidence on the localization of electrical responses in the cochlea, and it should be pointed out in this relation that Ruckmick fails to credit Hallpike and Smith with the discovery of this phenomenon.[6] It is perhaps to be assumed also in this connection that Ruckmick is not aware of the work of Crowe, Guild, and Polvogt on the localization of lesions in the human cochlea, for otherwise it would be difficult to defend his failure to mention this most outstanding contribution to the field of audition.[7] Other omissions, and additional errors that we do not mention, may possibly be excused on the grounds of limitations of time and space.

[5] E. G. Wever, C. W. Bray, and G. P. Horton, Tone Localization in the Cochlea. *Trans. Amer. Otol. Soc.*, 1935, **25**, 76–81, and also in *Ann. of Otol.*, 1935, **44**, 772–776.

[6] C. S. Hallpike and A. F. Rawdon Smith, The Helmholtz Resonance Theory of Hearing. *Nature* (London), 1934, **133**, 614.

[7] S. J. Crowe, S. R. Guild, and L. M. Polvogt, Observations on the Pathology of High-Tone Deafness. *Bull. Johns Hopkins Hospital*, 1934, **54**, 315–379.

IN REPLY TO WEVER AND BRAY

BY CHRISTIAN A. RUCKMICK
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The editor of the BULLETIN has kindly referred to me in manuscript form the response of Wever and Bray to my critical review of their work. At the outset it should be stated that I was very definitely instructed to make not a mere summary of results and conclusions in the auditory field but a *critical appraisal* of typical investigations in this difficult area of research. This change in attitude as compared with my previous reviews was clearly announced in the title and opening paragraphs of my recent article. Such an appraisal is naturally the fruit of the reviewer's own judgment and opinion and generalized statements are therefore never applied directly to the authors involved, but to an interpretation of their work. Even in the face of classical exceptions to the general rule, I never intended to argue *ad hominem*. Years ago Külpe personally told me that his seminar had just reviewed some of my experimental work on rhythm. "We did not entirely agree with you, but," he added genially, "that does not matter."

So much for the broader aspects of the situation. As for specific bases for some of the statements to which the authors take exception, let us take the items *seriatim*. As for (1), the data did clearly indicate to me the conclusion which was stated. It was drawn largely from the following passage quoted from the authors: "this present version of the Helmholtz theory can no longer be accepted; the theory must be subjected to further emendation or elaboration, or else be finally abandoned. At the same time, other theories of hearing are brought forward in more favorable light; and the demonstration of a correspondence between stimulus-frequency and frequency of response, together with the discovery of a response-frequency in the nerve as high as 4100~/sec. makes more reasonable than ever before the outstanding rival of the Helmholtz hypothesis, the so-called telephone theory of audition" (p. 349). Further support is afforded through a footnote reference to Boring's theory of audition which at least tentatively posits cortical dispersion in the summation of intensive quanta as opposed to qualitative discrimination at the specific sensory nerve endings in the cochlea.

The major opposition between resonance and non-resonance theories is therefore clear. If stress is placed on the correspondence between the stimulus-wave before the cochlea is reached and the wave picked up from the auditory nerve on the strong presumption that the latter wave is equivalent to what the cortex receives neurologically, then the cochlea transmits but does not analyze qualitatively in the sense of specific nerve energies.

Had Wever and Bray read my article as carefully as I read their publications in the first instance, they would not be led to wonder about the reference to 'contradictory results.' 'These' again definitely refers in the article to the literature in general which consists, as it happens, of reports from other investigators besides Wever and Bray. No serious scholar will deny that there are and have been contradictory results! The 'claims' are obvious from the above direct quotation at the end of the article referred to and from the tenor of a long line of further research in which Wever and Bray, either individually or collectively, have investigated 'hearing' by picking up electrical currents from auditory nerves.

(2) Now as for Fletcher's place in the controversy, I am amazed to discover how Wever and Bray have allocated to themselves the expression 'such results.' It is true that this paragraph follows the one referring to their investigations of 1930. But it also follows almost one-half of my critical review, to say nothing of contradictory results which have been noted in my previous summaries. Fletcher did nobly; but I presume that he would be the last to deny contradictory theoretical and experimental results, even before Wever and Bray first described their interesting technique. That the order of events was perfectly clear in the reviewer's mind can be seen from the fact that no errors occur in the dating of the bibliography to which he refers. 'Such results' obviously had to do with all the contradictory bits of evidence which already filled the literature in superabundance prior to 1930.

Another matter of chronology is equally fabulous. I did not 'make out' that Wever and Bray *first* lent their support to a combined theory in 1933. I said that "Wever has recently lent his support to such a compromise which he called the 'resonance volley theory,'" citing a 1933 article. But even if I had tried to discover when they first adhered to any theory, I could not have agreed with their present pronouncement because in the article which they now cite (1930), I can find no adherence or support whatever! After discussing "the present possibilities for auditory theory" and

describing in detail four such possibilities, they close with words nowhere in the article any stronger than these:

"We refrain at this time from expressing a preference for any one of these theories, and wish rather to look to further investigation for a decision. This discussion is made, not to propose or to defend a theory, but in the attempt to analyze the problem of audition in the light of evidence now available, and thus perhaps to converge further developments in the direction of a final solution."

What a 'support' was there, my countrymen!

(3) In regard to the systematic discrimination between physiological and psychological data which I am accused of 'repeatedly belaboring,' pray let me keep on 'belaboring.' No names were cited by me for academic distinction on this point and Wever and Bray have only themselves to blame for taking up the cudgels here. The methodological error is common to many fields of psychological investigation and is no error at all from the strictly behavioristic point of view. The subject requires more space than can be given here and I have discussed it at length in my latest book and in my presidential address to be published soon. Since I inveigh against no persons and do not believe that I anywhere directed this finger of warning against all who use the electrical techniques (since I also use electrical techniques!), may I quote the proverb, *qui s'excuse s'accuse*.

(4) This is a simple matter. In 1930 Wever and Bray did refer to "action currents in the auditory nerve in response to acoustical stimulation." They now no longer call their technique by that name. I am not confused on this subject at all because Travis and others employ this technique continually in our laboratories and I am quite familiar with the concept in actual use. Nor do I now learn for the first time that responses may be studied at various levels of the auditory system. My article is sufficient evidence of the fact that such studies are intimate acquaintances of mine. Confusion cannot obtain where knowledge is systematically and methodologically clear. Why should the statement that a term has been dropped in the course of further investigations be anything more than the notation of an historical event?

(5) The issue here consists in an interpretation of a purpose underlying research. Obviously experiments made under pathological conditions are not ordered merely to produce stimulation deafness. Certain facts bearing on the general problem of hearing become relevant. While the cautionary statement occurs in the article cited, we plainly read, just before that statement, the following passage:

"The results of this investigation agree with the conditioned response studies previously reported in indicating a general rather than a specific impairment of hearing as a result of stimulation with a single frequency of sound at great intensity. For the problem of localization in the cochlea, this seems to indicate no important degree of specificity for such tones. It is probable that when a tone attains an intensity of 100 *db.* or more, it involves practically the entire extent of the basilar membrane and allied structures."

The question raised in the frame of other investigations on the subject of general *vs.* specific function of the basilar membrane is: what exact bearing has this finding on "the problem of localization in the cochlea?" Can you make any comparisons at all with 'conditioned response studies' when you blast the mechanism with a tone of 100 *db.* for days on end? Wever is well aware that this objection was raised on the floor of the meetings of the A.P.A. at Ann Arbor when he read his paper—and was left unanswered.

(6) This requires no extended rejoinder. The defense of my tentative assumption is gathered from the literature as I have scrutinized it. The cochlea may function specifically in that tonal range which is most frequently used and which has been biologically most useful. The affair is my guess against that of Wever and Bray. These writers have already agreed that "a change in the refractory periods of the fibers during stimulation would not interfere with the synchronous character of the response, but would merely alter the total number of impulses in the response." I have allowed for the effect of volleys or summation-patterns by making the reservation that the fibers do not in this region (or any other for that matter) "transmit to the auditory nerve just what they receive."

(7) With respect to my failure to credit Hallpike and Smith may I again point out that my critical review was prescribed by the editors to include only typical pieces of research. Culler's work is at least as typical as the rest. The statement that I was presumably unaware of the work of Crowe, Guild, and Polvogt can be construed as nothing short of malevolent. If I believed that my two critics do their scientific work with as many unwarranted assumptions as they use to punctuate their discussion of my review, their work would deserve much less attention than it has received. As a matter of fact I had the material to which they refer before me when I wrote the review and it was in mind when I drew my deductions. It is also detailed in the symposium which is included in my bibliography and is mentioned in my article.

BOOK REVIEWS

WASHBURN, MARGARET FLOY, *The Animal Mind: A Text-Book of Comparative Psychology.* (Fourth Edition.) New York: The Macmillan Company, 1936. Pp. xii+536.

Ten years have elapsed since publication of the third edition of *The Animal Mind.* During this decade experimental comparative psychology has flourished as never before, and much has been added to our knowledge and understanding of animal behavior, particularly in the fields of learning and motivation. In this, the fourth edition, Professor Washburn has attempted to bring her book up to date by inclusion of this ten-year accumulation of new materials.

In comparing the third and fourth editions we find that the first eight chapters, covering methods, interpretation, and sensory discrimination, are practically unchanged. A short section on brightness discrimination has been added. The ninth chapter, on "Space Perception and the Perception of Things," has been revised and enlarged. Learning and other forms of "intelligent" behavior, to which 91 pages were devoted in the third edition, are now given 119 pages; the second of the two chapters dealing with these subjects is given a new caption: "Higher Mental Processes." Several topics, representing some of the more recent trends in research on these forms of adaptive behavior, are treated for the first time. An entirely new chapter, "The Affective Factors in the Animal Mind; Drives, Incentives, and Emotions," has been added. The thirteenth and final chapter, "Some Aspects of Attention," contains in almost unchanged form the materials presented in the last two (twelfth and thirteenth) chapters of the third edition. The current volume is distinguished from its predecessors by its bright green binding, by the larger number of pages (536 as contrasted with 431 in the third edition), and by the larger number of references (1,683 as compared with 1,135). The illustrations are the same as those used in 1926.

The principal change which has been made in the book, therefore, is the addition of new materials, particularly of results which have accumulated during the past decade. The general organization of the volume, its guiding viewpoint and interpretations, remain essentially unaltered. There has been a slight shift of emphasis from sensory capacities to complex patterns of behavior, dictated by the nature of

recent research. There are no new theories, interpretations, or syntheses of major importance.

Modernizing this well-known text, however, would have been in itself a most worth-while undertaking if the task had received the same painstaking and critical care which was accorded the earlier editions. Evidently and unfortunately this was not the case. The revision abounds in inaccuracies: misspellings, misstatements of fact, obvious misinterpretations, repetitions, and omissions. (This criticism, of course, refers only to the materials which have been newly added; not to the major portion of the book which has been taken over verbatim from previous editions.) A complete tabulation of errors is beyond the scope of this review, but the following may serve as examples.

According to a statement on page 234, Lashley found that rats could distinguish horizontal from vertical stripes "even when the width of the stripes was such as to subtend an angle of only 26°." This figure is clearly incorrect. In the article to which reference is made [1] Lashley says, "All pigmented animals . . . fail to discriminate when the lines are reduced to a width subtending about 57′ of arc at 25 cm." In a subsequent article,[2] which Washburn has omitted from the list of references but which obviously is the source of her statement, Lashley says that "the threshold of the pigmented rat for striae is below 52′ of arc and above 26′" (page 483). The name of T. L. McCulloch is misspelled "McCullough" six times, and in one instance the initials are given as "T. C." Tomilin (reference number 1402) is misspelled "Tomlin." Seven articles by Floyd L. Ruch have been ascribed to his brother, T. C. Ruch. There are serious omissions of reference, such as of Spence's 1932 monograph on the reliability of mazes in the specific discussion of this topic on pages 316–317. The index is incomplete and often incorrect. Figure 18, which on page 291 is referred to as the Stone multiple-light discrimination apparatus for rats is, instead, a ground plan of multiple choice apparatus used for pigs. No illustration of the Stone apparatus is included in the book. The statement, on page 291, "in 'trial and error learning,' which *should* be random" (reviewer's italics), is, without further qualification, definitely misleading. The first nine lines of section 88 are repeated exactly in the first nine lines of section 97. Description of an experiment by Fischel on page 391

[1] K. S. Lashley, The Mechanism of Vision. I. A Method for Rapid Analysis of Pattern-Vision in the Rat. *J. Genet. Psychol.*, 1930, **37**, 453–460.

[2] K. S. Lashley, The Mechanism of Vision. III. The Comparative Visual Acuity of Pigmented and Albino Rats. *J. Genet. Psychol.*, 1930, **37**, 481–484.

is repeated, almost word for word, on page 401. Although only an abstract was available, it is difficult to understand how Nissen's "ambivalent stimuli" experiment could have been so completely misinterpreted (pages 347–348). The discussion of "hypotheses" (pages 361–362) makes no mention of the relevant work and discussions of Yerkes, Hamilton, Lashley, and McGillivray and Stone. Even the beginning student of comparative psychology may wonder what "carefully standardized results" (page 394) are, and may well become sceptical when, following a listing of drives in the order of scores obtained in the Columbia obstruction apparatus, he reads that "*this order* produces scepticism" (reviewer's italics).

It is in a sense unfair to judge the merits of a book of this type in terms of its more elementary formal characteristics. It should be possible to take such matters for granted and to base one's evaluation on the more significant aspects of viewpoint, emphasis, organization, comprehensiveness, clarity, and originality. The carelessness with which the most recent edition of *The Animal Mind* was prepared, however, cannot but detract seriously from the usefulness of the book and, one fears, will diminish rather than augment the esteem in which it has been held.

The first edition of *The Animal Mind* was published in 1908. The second edition appeared in 1917, the third in 1926, and this, the fourth, in 1936. During most of this twenty-eight-year span Washburn's volume has been almost the only work in its field in English which could lay claim to any degree of comprehensiveness. Although Watson's *Behavior* was used widely as a text following publication in 1914, this book emphasized methodology and principles and was more narrowly limited in factual content. Within the last three years there has been a rapid multiplication of texts available in the field,[3] of which only one, however, the *Comparative Psychology* of Warden, Jenkins and Warner, clearly exceeds *The Animal Mind* in scope. This sudden increase in the number of textbooks and handbooks of animal behavior suggests the venture of some tentative speculations regarding the place of *The Animal Mind* in comparative psychology during the many years when it was almost alone in the field.

[3] N. L. Munn, *An Introduction to Animal Psycholog: The Behavior of the Rat.* New York: Houghton Mifflin Co., 1933. Pp. xxii+439; F. A. Moss (Ed.), *Comparative Psychology.* New York: Prentice-Hall, Inc., 1934. Pp. xiii+529; N. R. F. Maier and T. C. Schneirla, *Principles of Animal Psychology.* New York: McGraw-Hill Book Company, 1935. Pp. xiii+529; and C. J. Warden, T. N. Jenkins, and L. H. Warner, *Comparative Psychology.* (Vol. I) *Principles and Methods.* (Vol. II) *Vertebrates.* New York: Ronald Press Company, 1935 and 1936. Pp. x+506 and x+560.

Boring, in his *History*, describes *The Animal Mind* as "a very thorough handbook of all research in the field." Although one must agree that it was long the most inclusive summary of experimental work available, it is obvious that Miss Washburn did not attempt to cover all aspects of comparative psychology. (This fact is explicitly stated in the preface to the first edition.) At no time, for instance, has her volume given more than cursory treatment of the immense body of naturalistic and semi-experimental data on instinctive patterns of behavior. Investigations of the neurological correlates of behavior have been entirely neglected. Studies of social behavior, of the native *versus* acquired behavior problem, of motor capacities and characteristics, and, until the present edition, of dynamic or motivational factors, have received but scant attention. It may be, and by some certainly will be, considered one of the chief merits of the book that it confined itself almost entirely to strictly experimental results of behavior studies, and that it virtually ignored both the perplexing and usually fruitless instinct controversy and the morphological and physiological determinants of behavior. The very fact of these omissions has doubtless been of influence (whether salutary or otherwise may be open to question) on the development of comparative psychology.

It is, on the other hand, not entirely correct to call *The Animal Mind* a "handbook." It is more than that. For in addition to giving a summary of factual knowledge, it has made important original contributions, of which the learning theory with its hypothesis of a "gradient" of excitation of the drive stimulus, the chapter on methods of studying discrimination, and the system of classification for types of behavior modification, may be cited as examples. The importance of such additions to organization and theory are even today too apparent to require further elaboration.

The criticism which probably is most frequently leveled against *The Animal Mind* is its alleged anthropomorphism. It seems to this reviewer, however, that the mentalistic flavor of Professor Washburn's book, which is perhaps even more pronounced in 1936 than it was in 1926,[4] need cause the objectivist no serious concern.

[4] In the preface to the fourth edition the author writes: "The principal change in the attitude of investigators of animal behavior since the third edition of this work appeared is the decay of behaviorism as an interpretation and the revival of animal psychology. The conclusions drawn from experiments are now expressed in subjective terms. Undoubtedly one cause of this change has been the rise of the configurational school. But extreme behaviorism, which ignored the existence of all qualitative differences in sensations, would not have long endured" (p. v).

Her account is for the most part thoroughly objective, so that, with a few changes in terminology and the omission of certain paragraphs, *The Animal Mind* might appropriately be renamed *Animal Behavior*. (It is not assumed, of course, that Miss Washburn would at all agree with this "defense.") Speculations concerning the inference of consciousness are often tacked on at the end of chapters and, with the possible exception of their effects on especially young and tender readers, are probably as harmless as they are useless and extraneous to the main exposition. Only rarely are conscious processes given the status of causes and, in these isolated instances, quite unnecessarily. Thus, in her excellent *Movement and Mental Imagery*, Professor Washburn herself has given us a satisfyingly objective account of the memory image which plays so large a part in her exposition of the higher mental processes. The treatment of attention would require only minor changes here and there to make it acceptable to the run-of-the-mine behaviorist. The statement (p. 210) that "we are not abandoning mechansim in favor of vitalism" is incorrect in suggesting that the book represents a mechanistic viewpoint, but is accurate in implying that the author does not resort to vitalism and, scarcely at all, to teleology. The expression, "dropping out of useless and harmful movements," for instance, serves strictly descriptive purposes and is not exploited as an explanation. There is in the fourth edition some tendency to humanize animals ("Another symptom of the higher opinion of the animal mind that is now prevalent" p. 361), but this inclination is never sufficiently strong to interfere with conservative interpretation. It may be said in general, therefore, that Dr. Washburn's volume definitely belongs to, and doubtless has itself advanced, the American trend towards objective description and away from mentalistic or vitalistic explanation.

Whether *The Animal Mind* should also be charged, or credited, with the development in America of comparative psychology as an adjunct to human psychology on the one hand and to physiology on the other, rather than as an independent branch of biological science, is perhaps open to question. It has not, at any rate, taken full advantage of its opportunities in this direction, and this we may consider regrettable or fortunate according to our interests. There is, however, no doubt that Professor Washburn has rendered invaluable service in providing for these many years a comprehensive survey of experimental studies of animal behavior from which, we may assume,

many original investigations, theoretical treatments, and even bibliographies have taken their start.

Yale University. HENRY W. NISSEN.

BRUNSWIK, EGON, *Experimentelle Psychologie in Demonstrationen.* Wien: Julius Springer, 1935. Pp. x+166.

It is a fairly simple matter to describe in detail the contents of this book as it would be, also, of Titchener's *Qualitative Manual,* but the reviewer's task of evaluation in this instance is a different and more difficult job. It is next to impossible to evaluate from a detached point of view, and when one has an inadequate understanding and appreciation of foreign concepts, one's judgments of those concepts are apt to be as unreliable as those of a religious mystic would be upon dialectical materialism.

This review will give a brief statement of the purpose of the book, then a more extensive description of the contents followed by a brief criticism or evaluation.

The book is designed as both a text and laboratory manual or guide for university courses in psychology, for teachers of philosophy in "middle schools" and for those non-professionals who may wish a treatment of experimental psychology "in its present state of development." The "construction of the book is systematic." The materials are chosen to show "a basic uniformity of pattern" and convincing public demonstrations. Accordingly, there is a preponderance of sense-perception material especially in the field of vision. The "trend of the experimental inquiry makes possible a treatment of the really serious and important questions without many complicated pieces of apparatus." Aside from a balloptican with time-control of projection, a color-mixer and a stop-watch suffice for the majority of the demonstrations.

In complete form the course outlined in this book should take two semesters with two to four hours weekly, but it may be reduced without serious encroachment by the elimination of some of the more time-consuming experiments.

There are seven sections covering the following topics consecutively: Sense Qualities, Form Perception, Space and Size Perception, Memory and Thought, Feeling, Action and Personality Types. If one may judge by the space allotted them, the topics vary greatly in importance, for 106 pages or two-thirds of the book are devoted to the first three sections, to those subjects for which the Gestalt theory and technique is best adaptable. In the first section on Sense Quali-

ties, for example, there are 29 pages, 28 of which are concerned with after-images, color-contrast, color-blindness, color-mixing, color-systems, threshold and Weber's Law and light and dark vision. The one remaining page considers "the arrangement of other sensory fields"; hearing, smell, taste, and cutaneous senses. The second section of 42 pages on Form Perception includes eight chapters on visual form perception with 12 pages on geometrical-optical illusions. Nothing is said of tactual and kinesthetic form-perception. Likewise the third section on Space and Size Perception is exclusively visual.

The last four sections, on topics to which the majority of the space in modern American texts is given, are very brief; especially Section V on Feeling which compresses four chapters into five pages.

Memory and Thought are considered in Section IV. Memorizing methods and various measurements of learning and retention are well, though too briefly, covered in six chapters. Thought is discussed in two pages. It is interesting to note that the author presents the theory of visual, auditory, and motor thinking types.

The section on Feeling is confined largely to discussion of pleasantness and unpleasantness rather than experimental demonstrations on feeling and emotion. The problem of stimulus-intensity and feeling is considered briefly. In Section VI on Action, the subjects of reflex and voluntary action are covered in two pages. Several brief tests and demonstrations are suggested on specificity in action systems.

Brunswik is a consistent follower of Kretschmer in regard to Personality. Section VII is entitled Personality Types and the four chapters in the section deal with "Self Diagnosis," "Physical Types," "Expression and Personality," and "Experimental Typology." The argument in Chapter Four (Experimental Typology) runs as follows: Cyclothymes and Schizothymes are differentiated in several ways; for instance, the Cyclothyme is a "color-seer" while the Schizothyme is a "form-seer." The Cyclothymes are superior in voluntary mobility. The Cyclothyme is more practical while the Schizothyme is more symbolic in his thinking. The Schizothymes have a greater capacity to divide their personality in accordance with different situations without changing their basic personality. In short, they are better actors. The Cyclothymes can adapt their basic personalities in accordance with environmental conditions. The Cyclothymes are centrists while the Schizothymes are extremists. There is more exposition of this sort but a lack of evidence or demonstrations which might be expected under the topic of Experimental Typology.

Elementary statistical methods are suggested in connection with certain demonstrations, particularly in the first part of the book. Compared with typical modern American texts or manuals in experimental psychology, the book presents a very limited bibliography. Only 31 titles are listed, 16 texts and 15 articles, dating from 1913 to 1934. All titles are in German and all are based, apparently, on the standpoint of Gestalt theory.

In evaluating this book as a text in experimental psychology, the reviewer wishes that he could refer to one or two reviews by German colleagues of Brunswik. It would be better to see the book in the light of those who use it. It is very probable, of course, that it is considered to be a substantial presentation by many psychologists in German and Austrian universities, but, in the opinion of this reviewer, it is inadequate in scope and gives a false picture of the present status of experimental psychology. It is not up to the standards of the early European experimentalists nor their American students. One would not suspect that there had been any contributions to experimental psychology, except under the aegis of Gestalt psychology. The considerable attainments of physiological and materialist psychology in the fields of sensation, emotion, action, and individual differences certainly belong in any course on experimental psychology and many of these attainments are demonstrable in such a course. Many of the demonstrations in this book are extremely valuable and nicely adapted but they are illustrations of what may be considered a retreat to subjective psychology which is a frustration of science rather than an exposition of scientific method and achievement.

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MORGAN, J. J. B., *The Psychology of Abnormal People: With Educational Applications.* (Second Edition.) New York: Longmans, Green and Company, 1936. Pp. vii+605.

In the review of the first edition of this text (*Psychol. Bull.*, 1929, 26, 620–622) it was pointed out that Professor Morgan had written a descriptive psychology of the abnormal personality at the general textbook level. Seven years of use indicate that Professor Morgan's presentation has been satisfactory to many teachers of abnormal psychology, since this book has been one of the most popular texts in the field.

Morgan states in the preface of the second edition that he has

attempted to give proper weight to all significant recent research in the field of abnormal psychology and to increase the clarity of the presentation. He has changed the title of certain chapters of the book, viz., " Hysteria " is now called " Benign Mental Disorder," while " Mental Hygiene " is now called " Treatment and Prevention." Other than these changes in title, the method and organization of presentation is essentially that of the original edition. He has followed the more conservative psychological classification into disorders of sensation, perception, association, memory, emotion, etc. The usual chapters on sleep and hypnosis are included.

With respect to the clarity of presentation it seems to the reviewer that this second edition is even more clearly presented than the first edition. It should be easily comprehended by the average college student and form the basis of a satisfactory presentation for the conservative type of course in abnormal psychology.

Professor Morgan states that he has attempted to include the more significant recent research in this field. There has been an immense amount of really fundamental research carried on since 1928 which bears on the experimentally determined foundation for the study of personalities, both normal and abnormal. A careful study of the chapter on personality shows practically no new references or general discussions added to this chapter which would indicate that any progress whatever has been made since 1928 in this most important aspect of abnormal psychology. Surveying other chapters in a similar fashion shows that in some instances relevant new experimental material has been added but in other cases the experimental world of research has not been productive of any findings which Morgan has felt worth including.

In general, then, one can say that this second edition of a successful text is clearer and should be more usable in classroom work. However, it is disappointing in that it does not indicate the inclusion of real experimental findings which do exist and which should be made available to the beginning student.

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BOOKS RECEIVED

BACHELARD, G., *La dialectique de la durée.* Paris: Boivin et C^ie^, 5 Rue Palatine, (VI^e^), 1936. Pp. 170.

BRUNSCHWIG, L., *A Study of Some Personality Aspects of Deaf Children.* T. C. Contr. to Educ. No. 687. New York: Bureau of Publications, Teachers College, Columbia University, 1936. Pp. xi+143.

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PORTO, C., *L'instinct.* Paris: José Corti, 6 Rue de Clichy, 1936. Pp. 285.

RUNDQUIST, E. A., and SLETTO, R. F., *Personality in the Depression.* Minneapolis: The University of Minnesota Press, 1936. Pp. xxii+398.

NOTES AND NEWS

DOCTOR SAMUEL J. BECK has been appointed in charge of the Psychology Laboratory in the Department of Psychiatry, Michael Reese Hospital, Chicago. Doctor Beck has been in the Department of Psychiatry in the Harvard Medical School, and the Psychology Laboratory of the Boston Psychopathic Hospital, Boston, Massachusetts, where he was research associate.

THE fourteenth annual meeting of the American Orthopsychiatric Association will be held at the Roosevelt Hotel in New York City, New York, February 18 to 20, 1937. The president of the society is Dr. Edgar A. Doll, and the secretary is Dr. George S. Stevenson, 50 West 50th Street, New York, New York.

AT THE last meeting of the American Psychological Association at Hanover, it elected a representative to the Board of Directors of the New York Management Council, which is affiliated with the National Management Council. The New York Council is an informal organization through which local societies and the local sections of national societies concerned with various aspects of business and industrial management, coöperate in promoting the mutual interest of their members.

Nine meetings have been scheduled for 1936–1937, to be held in the auditorium of the Metal Products Exhibits, International Building, Rockefeller Center, at 7:45 P.M. Registration fee to members of participating societies, 50 cents.

Sept. 15. James O. McKinsey, Chairman of Board of Marshall Field & Co., "A Challenge to Management."

Oct. 6. Walter D. Fuller, President of Curtis Publishing Co., "Business Management Today."

Nov. 12. F. Alex. Magoun, Mass. Inst. of Tech., "Men, Management and the Future."

Dec. 8. Frank R. Coutant, President American Marketing Society, "Discovering and Developing Markets."

Jan. 7. Harry Arthur Kopf, Deputy President International Committee on Scientific Management, "The Office in Business and Industrial Management."

Feb. 9. Charles F. Roos, Director of Research, Cowles Committee of Research in Economics, "Expected Contribution of Economic Theory and Measurement to Management."

Mar. 9. Saunders Norvell, Chairman of Board of National Federation of Sales Executives, "The Human Side of Business."

Apr. 6. John T. Briggs, Secretary of N. Y. Society of Architects, "Layout for Income."

May 4. C. Canby Balderston, Wharton School of Finance and Industry, University of Pennsylvania, "Profit Sharing."

THE postponed Eleventh International Congress of Psychology will be held at Paris, July 25–31, 1937. The Honorary President will be Professor Pierre Janet. Professor Henri Piéron is President of the Organization Committee and the General Secretary is Professor I. Meyerson. The headquarters of the Congress are at the Laboratory of Psychology of the Sorbonne, Paris (5).